

ALBERT WOHLSTETTER



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Dear Joshua,

Thanks very much for the offprints on Ithiel De Sola Pool and Bernard Cohen, both of whom I knew, Ithiel in the 1950s and Bernard in the late 1930s and early 1940s. (I lost touch with them and hadn't heard of the two books you called to my attention.) I am curious to look at them now. I've mislaid the review of Bernard's book. Could you resend it to me?

I've been meaning to write you about them and especially about J.H. Woodger, whose work on the axiomization of biology played a substantial role in some parallel attempts I was making of an even more ambitious sort when I was a young mathematical logician. I knew Woodger and, like yourself, had a high regard for him and his work in axiomatics. In fact, I reviewed his monograph on "The Technique of Theory Construction," at Alonzo Church's request, for the *Journal of Symbolic Logic*. (For your entertainment I am enclosing my review.)

At the time I had received a fellowship for the application of methods of modern mathematics to theory and measurement in economics. But economics I found to be a much harder test than biology for the fertility of axiomatics in deducing "unsuspected consequences," and especially for guiding experimental, or at least empirical, inquiry. I did an axiomization of utilities as a partially ordered field, which was of lasting value for me in dealing with the choices among chancy and highly uncertain policy alternatives. Policy alternatives with uncertain consequences are not all subject to even the weak ordering in which one could say that Policy A is no worse than Policy B. Some were indeterminate, and part of <sup>the</sup> purpose of systems design is to construct some clearly better alternatives which, incidentally, extends the field of partial order. The axiomization of utilities as a partially ordered field formed the basis for my focus on *a fortiori* reasoning, on very gross inequalities rather than equations, and on systems design rather than systems analysis. I continue to find that axiomization fruitful.

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However, empirical macroeconomic theory, I was finding at the time of my review of Woodger, was something else again. Naturally I came to it full of enthusiasm inspired by the revisions of fundamental theory in relativity and quantum physics that physicists had made as the result of careful attention to the basic theoretical implications of the limits of subatomic and astronomical measurements. Nonetheless, I was stunned when I came to understand the formidable problems of measurement in economics. This stems not only from the characteristic paucity of data compared to the situation in physics, but from the substantial intrinsic indeterminacy of aggregative economic concepts.

Such indeterminacies, for example, arise from the fact that, to measure changes in real gross national product or in productivity over an extended time period, one has to face the fact that some products and services at the start of the period are not available at its end, and vice versa. For that reason alone it is not possible to separate fully the real physical changes from those that result from changes in unit prices. Moreover, the differences are not at all trivial. That emerges in any serious attempt to understand, for example, trends in productivity: A key aspect of invention, innovation and advance in productivity is precisely the creation of new products and new services. The information revolution offers some of the best examples where it's clear that simple statistical maneuvers that attempt to get around this basic indeterminacy merely throw the baby out with the bath water.

I came to feel that one of the enterprises I had undertaken -- the axiomization and formalization of various received macroscopic economic theories, such as those of Keynes and Marx -- was a little like trying to dig the Suez or Panama Canals with a scalpel.

Later, when I was doing quality control in a factory and wanted to recall some elementary physics, for the fun of it and as one way of recalling enough mechanics to argue with the engineering department, I tried and did succeed in axiomatizing classical mechanics. That was done later, better and published by my friend J.C.C. McKinsey. Classical mechanics, I believe, was susceptible to such rigorous axiomatic treatment in good part because it was finished and quite dead, with no important change since the 18th century.

My review of Woodger's monograph, you'll notice, ended by my indicating that Woodger's very clean-cut and able application of modern logical theory nonetheless left me with some doubts, which I was not competent to resolve, about its biological importance. Woodger's work obviously had other values. But I have long wanted to ask you whether the axiomatic

method in biology ever did result in the deduction of "unsuspected consequences" and useful "guidance of the experimenter."

I've been enjoying -- intermittently, when the crashing of my computer, the collapse of our ceiling while a new roof was being put on, and the urgencies of the slaughter in Iraq and elsewhere have left me the time -- trying to reconstruct how I arrived at my current intellectual position. It's a good time to get perspective.

Meanwhile I enclose a longish piece on Iraq and the intrinsic connection between the need to stop the regime's terror against its population and preventing it from reviving its capacity to terrorize its neighbors. I hope you will recognize in it some of your own views about the importance of openness to make arms control for biological and chemical weapons feasible.

Warm regards,

A handwritten signature in black ink, appearing to be 'R. A. Woodger', with a large loop at the start and a long horizontal stroke at the end.

encl: AW review of Woodger, from *JSL*, March 1940  
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